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ABSTRACT

Perhaps due to misunderstandings of the nature of the research task in visual communication, findings of empirical research in that field have been highly inconsistent. Historically, there has been little serious systematic empirical investigation of visual communication. One difficulty is that it is not always possible to measure nonverbal responses nor to know precisely what caused them. It is also not clear what measures are relevant in the field nor precisely what the domain for research is. Several research designs are possible in visual communication research: factorial design (requires many subjects and is relatively insensitive), repeated measures design (allows for intersubject variability and is fairly sensitive but may confound the effects of the messages and the interaction between subjects and messages), and multiple variable studies. An eclectic, initially descriptive science of visual communication that defines its area of concern with sensitive, reliable, and relevant measures is called for. (TJ)

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EMPIRICAL STUDIES OF VISUAL COMMUNICATION:

SOME METHODOLOGICAL CONSIDERATIONS

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Overall, one cannot be much encouraged by the accumulated results of empirical studies in visual communication.[1] Even the simplest concepts -- camera angle, asymmetry, color -- have produced such inconsistent findings that the casual reader must marvel at the persistence of investigators in this field. This research morass may be due in part to misunderstandings as to the nature of the research task in visual communication.

The most direct formulation of the behavioral research task is that the phenomena of interest can be described in four dimensions -- person, stimuli, responses, context.[2]

Insert Figure 1 about here.

Studies of visual communication are concerned primarily with stimulus variables within the frame of a projection screen or television tube. The image on the screen appears as part of a string of temporally related images in a scene before the camera, an element of a message or program form or formula.[3] The screen appears within the environment of the viewer, an environment influential in determining the viewer's response to the image within the frame.[4] The effect of image or environment will be reflected only in the response pattern tapped by our measurements -- retention, attention, attitude -- and filtered by the response tendencies which characterize the viewers being measured. The screen manipulation made by the investigator will explain very little of the total variance within the viewing situation at any moment. Expressed in algebraic terms:

$$\sigma^2 + \sigma^2_{\text{message}} + \sigma^2_{\text{program form}} + \sigma^2_{\text{viewing environment}} + \sigma^2_{\text{within subjects}} + \sigma^2_{\text{measurement error}}$$

$$= \sigma^2_{\text{viewing situation}} .$$

Separating the effect of an image manipulation will require very sensitive measures as well as controls and statistical routines to isolate the relatively small part of the total variation assignable to the visual image.

Insert Figure 2 about here.

In view of the above empirical studies in visual communication require (a) an unambiguous image manipulation, (b) a controlled viewing environment, (c) a controlled program environment, (d) an assessment of viewer response tendencies and (e) sensitive measures of known reliability and composition.

Background

Some of the earliest empirical investigations of visual communication variables were studies of film sponsored by the Payne Fund and others in the thirties.[5] The objectives of these studies had more to do with the behavior of patrons of the cinema than with the evolution of an empirically based aesthetic for the screen.

Instructional television research burgeoned in the late fifties and early sixties with impetus provided by liberally given government grants.[6] The aim of most of these studies was to demonstrate the superior qualities of television as an instructional medium. As a consequence, these studies did not always systematically isolate the effects of visual communication variables, and, when they did, often used very rough dependent measures (as is characteristic in studies of instructional method). Few of these results are generalizable.

In many studies of advertising and political campaigning messages which differ along some visual dimension are contrasted by sponsors and advertising agencies. Unfortunately (a) these studies are rarely available for scholarly examination due to the competitive advantage that might be lost thereby, and (b) the studies available indicate that the image manipulations have rarely been systematic.

Considerable attention has been given to the so-called "program doctors" of local television news programs -- research firms called in to discover strengths and weaknesses in these programs and to improve ratings. The research of program doctors is rarely made public and almost never involves systematic manipulation of the image variables.

In sum, in spite of at least passing interest from scholars and scientists for more than four decades, the age of serious, systematic

empirical investigation of visual communication, is still before us.

The Nonverbal Problem

Empirical study of visual communication is distinguished by concern with essentially nonverbal message elements and (for the most part) nonverbal responses in the audience. By way of contrast, studies in rhetoric and public address may be concerned with nonverbal message elements (such as gesture or facial expression) but with verbal responses from the audience (such as retention, recall or attitude as measured by a Likert scale). The temptation to cast studies of visual communication into the mold of the "new" rhetoric is to misunderstand the nature of the phenomena being studied.

The essentially nonverbal responses of concern in studies of visual communication include attention, interest, liking, positive and negative affect, dominance, beauty, and others. These responses may be consummatory rather than instrumental, at least insofar as the study setting is concerned.[7] Students of visual communication are not so concerned with delayed effects such as voting or buying as they are with the immediate influence of screen image upon perception and comprehension. At the same time consummatory responses to visual communication often are not easily observed and require sensitive measures. And the difficulty in instrumenting consummatory responses suggests that many easily observable responses may conceal or confound the underlying consummatory responses.

Experiments -- Sacred Cows or Bum Steers

Perhaps in misplaced loyalty to "scientific" rigor, experimental strategies and designs have been adapted to study of visual communication

without enough consideration of the benefits and hazards. The logic of the experiment is that a number of variables involved in a phenomenon are held constant or controlled while one or more other variables are systematically varied and still others measured. The variables which are systematically varied are called independent measures, while those being measured are called dependent measures. The purpose of the experiment is to document the relationship between independent and dependent measures.

In the case of visual communication, the space in which the phenomena of interest occur is very poorly defined. We do not know very much about which measures are relevant in this space, either as independent or dependent measures: "How can you get to Paris if you don't know where you are? Should you try the train, plane or boat?" Since we do not understand the relevance of very many measures to the process of visual communication, it may not be appropriate to employ a study method -- the experiment -- designed for a better defined domain of investigation.

The problem may be more clear if we consider only that part of the visual communication process which is contained within the frame or mask of the two dimensional screen -- of film or television. What characteristics of this screen are unambiguously defined at present? One might answer, the height of the screen and its width. It is true we can unambiguously measure the height and width of the screen with a ruler or tape measure. And knowing these dimensions, we can also describe the aspect ratio of the screen. But, is it not true that these dimensions and aspect ratio represent only one limit of the dimensions of the frame? An iris shot in film or a keyhole split screen in television introduce a smaller screen inside the frame -- a frame within a frame -- possibly with a different aspect ratio. And what is the effect of shooting in limbo -- with a figure against a black background. Could it

be to bring the frame down to the outline of the subject? Or could it be to eliminate the frame from the perception of the audience? And what of the relationship in television of the mask of the home receiver to the frame on the program monitor in the control room of the studio? Home receivers are notoriously out of alignment, meaning that the composition on the screen does not always resemble the composition perceived by the director.

These questions in operational definition of the frame are answerable, but we are only beginning to provide answers which are defensible in empirical terms. For even these simple variables -- frame size and aspect ratio -- the implications of our inexactitude are considerable. Without precision as to the frame, for example, principles like Zettl's "magnetism of the frame" and symmetry/asymmetry cannot be empirically studied.[8]

Experiments then may not be possible or appropriate study frameworks for a field of investigation in which so little has been done to identify critical variables of interest. What must preclude extensive experimental investigation is empirical identification and development of reliable, sensitive and relevant measures in order to define and delimit the phenomenal space of visual communication.

On the other hand, the techniques of experimental study are very valuable in this process of definition and delimitation.

Study Design

The design of an empirical study in visual communication -- as in other fields of empirical study -- deals with the way in which test messages are presented, the ways in which subjects are selected to be exposed to

the various messages, the ways in which response measures will be taken, and the analytic routines that will be employed in understanding and interpreting results.

In virtually every field of communication research the most frequently encountered study design is the factorial design. In a factorial design subjects are randomiy assigned to various sub-groups of equal size. Each of the sub-groups is then exposed to a different test message. The resulting dependent measures from each group are compared between groups.[9]

Insert Figure 3 about here.

The factorial design accommodates the differences in response patterns among subjects by randomly assigning subjects to each troup. Whether or not random assignment actually distributes inter-subject differences equitably is largely a function of the size of the sample involved. Hence the larger the total number of subjects, the better the factorial study involved.

But the relatively large numbers of subjects required is not the only expense involved in factorial designs. Another is the relative insensitivity of the design. The size of measurement error -- variability in measurement due to inaccuracy in the measure -- in this design is dependent upon the size of each sub-group rather than upon the total number of subjects in the study.

Many users of factorial study designs in visual communications feel it necessary to use a control group. The purpose of control groups is to account for variability in the measures due to features of the study other than the planned manipulation in the visual images of the study. Sometimes the intent of the investigator may be to create a baseline of measurement

for comparison purposes by showing one group of subjects a message in which the visual element being studied is not present. If this is an effort to create a zero condition, it probably is not necessary, as our ability to quantify visual variables is too fragmentary. If we cannot describe the manipulations we do make, we are at the same disadvantage in describing the manipulations we do not make.

Much to be preferred over factorial designs in most visual communication studies at this stage in the development of the field are the repeated measures study designs.

Insert Figure 4 about here.

In a repeated measures design the same messages are presented to all subjects in the study: the subjects are not divided into separate sub-groups.[10]

No assumptions are made in this design about the distribution of intersubject variability. Intrasubject variability across the study messages is accounted for statistically, before the overall effect of each message is assayed. This statistical procedure permits the measures to be more sensitive to differences measured in responses to the various images. Again both sampling error and intersubject error disappear in this design.

The size of the error in measurement associated with this design is smaller in relative terms than in a factorial design with the same number of subjects, since all of the subjects of the study considered together establish the size of the error of measurement term.

Certainly there are problems in the use of repeated measures designs. One frequently identified problem is that of homogeneity of covariance.[11] The covariance in question is that among responses to the test messages --

covariances between subject responses to each possible pair of test messages. Figure shows the two conditions -- homogeneity of covariance and heterogeneity of covariance.

Insert Figures 5a and b here.

Homogeneity of covariance is the desirable situation; heterogeneity of covariance is not. Should heterogeneity of covariance characterize the measurement data of a study, then the resulting inferential test statistic (an F test, for example) is biased in that it confounds the effects of the test messages and the interaction between subjects and messages. In a one-way repeated measures design, of course, the interaction of subject and messages is not directly assessable, yet the possible presence of such an interaction could affect the data. Such subject-message interactions have both statistical and theoretic significance. In studies of learning, for example, such interactions are known as "aptitude-treatment interactions" and have spawned an extensive literature.[12] The statistical significance of such interaction is the bias of test statistics referred to here.

There are a number of ways of coping with possible heterogeneity of covariance in repeated measures studies. One is to design the study so that there are only two or two classes of test messages for comparison. This does not eliminate possible interactions between stimuli and subjects, but it does keep the effect of such interaction out of the resulting data.

Another approach to the problem is to imagine the worst possible bias in the F test of the repeated measures analysis of variance by testing the F computed from the data against a criterion F on 1 and N-1 degrees of freedom, as well as against a criterion F on 1 and a-1 degrees of freedom (where N is the number of subjects and a is the number of different test messages). If

the computed F is significant against both these criterion Fs, then any ill effect of heterogeneity of covariance in the data is not manifest in the measurements.[13] If the computed F is not significant against both these criterion Fs, then an exact F test, Hotelling's T^2, can be computed.[14] These precautions are computationally simple and available in most computer statistical packages designed for the behavioral sciences.

Still another matter of concern in connection with repeated measures designs is balanced presentation of stimuli. One of the few well established principles in communication study is that order of presentation affects the influence of messages upon audiences.[15] As a consequence, it is desirable that an equal number of study subjects be exposed to the study messages in each possible order in which the messages could be presented. If only two messages are involved, half of the subjects would be exposed to Message A first, the other half to Message B first. The data from the two sample halves is merged for analysis as one body of data.

If the number of study messages is large, this requirement for balanced presentation may impose a considerable burden upon the investigator, as the number of possible order combinations of a finite set of study messages is N(N - 1). With as few as five messages, the number of possible order combinations is twenty. A compromise method for balancing presentation of study messages is to select at random possible order combinations in which each of the study messages appear in first position and in last position. The exact impact of such a compromise cannot be estimated precisely in advance, but it can be assessed after the data has been collected by distributing the data as an analysis of variance in which each order of presentation is considered a level of treatment. If a

significant F test results, then additional data would have to be collected and the previously unselected order combinations presented to additional subjects, so that all possible order combinations will be presented in the study as a whole.

All things considered, the prudent investigator, at this stage of the development of empirical study of visual communication, will keep the number of messages in any study small so that he can reasonably present them in all possible order combinations.

Multiple Variable Studies

The foregoing argues for simpler designs for empirical studies in visual communication, since the variables of interest are not yet well defined by the available measures. The corollary of this assertion is that more effort ought to be put to identifying those measures potentially useful to an evolving study of visual communication.

Recommended are studies in which a variety of measures are simultaneously brought to bear: these will be referred to here as multiple variable studies to contrast them with multivariate studies. Multivariate techniques, as the term is used here, are statistical procedures by which combinations of measures provide insights into underlying relationships not directly apparent in any of the measures considered singly. Some of the frequently encountered multivariate techniques are factor analysis, multiple regression, multiple discrimina analysis, canonical correlation, and path analysis.

A multiple variable study is a more general class of study, one with more than one measured attribute. The multiple variables in such a study may or may not be subjected to multivariate analytic techniques. But the

ability of each measure to discriminate subject responses to the study messages is the important question in such studies. For visual communication studies, the specific measurement attributes of a measure that demand analysis are (a) sensitivity, (b) reliability and (c) validity.

Sensitivity. The general case for sensitivity in measurement in studies of visual communication has already been made. A sensitive measure is one which assigns numbers to the magnitude of some response dimension and does so in an unambiguous way. If we have produced two visual messages so that they differ on the same image dimension, then a sensitive measure of a subject's perception of these two messages will show a consistent quantitative difference in perception. Let us suppose that the kind of perception that is of concern in a particular study is "interest value" and that the test messages are two public service announcements for television. In one of the study messages all of the shots are joined by cuts only; in the other shots are joined by twelve-frame-dissolves only. (Knowing the effect upon interest value of these two editing devices might be important in production management, since (a) cuts are cheaper than dissolves when working in film and (b) the very length of dissolves may call attention to themselves. Many other propositions might also be relevant.) Our subjective experience as viewers may tell us that the difference in perception of two messages which differ only in transition devices will be small. This problem of quantification amounts to saying that discrimination of the responses to these two messages is a matter of scaling the two stimuli on the same response dimension, with enough distance between scale values that measurement error does not obscure the distinction between the two values.

Scaling stimuli is the object of classical psychophysics.[16] If an investigator is uncertain of the sensitivity of some measure of human

perception well established scaling procedures from psychophysics, such as rank ordering[17] or paired comparison,[18] will produce unambivalent discriminations.

Attitude scales are multiple rating scales that are refined by eliminating scales that do not discriminate (Likert procedure[19]) or by grouping scales that seem to reflect common underlying variables (semantic differential[20]). These procedures are sensitive to scalar distance and, with refinement in series of studies, can become very sensitive.

Reliability. The sensitivity of a measure is not interpretable if the measure does not produce replicable results. A reliability coefficient reflects the correlation coefficient between a theoretic error-free measurement score and the actual measurement score computed from the sample. Two well recognized routines -- test-retest and internal consistency -- are available to estimate the reliability coefficient of a multiple scale or multiple item measure.[21]

Validity. Validity -- whether a measure reflects the attribute intended -- is always the most difficult quality of a measure to assess. This will be particularly so in studies of visual communication, since very few of the theoretically significant propositions for empirical test have evolved from empirical findings. We don't know what our measures ought to measure. Under such circumstances, deductive logic becomes hit-or-miss. Inductive patterns of generalization are not so hit-or-miss, but promise less impressive theoretic gains.

Consider again the case of visual asymmetry. At the absolute level it is a simple matter to specify symmetry: if right half of screen is the mirror image of left half, then the image is symmetric. But if the image

is not symmetric, how asymmetric is it? At the moment, the available measures fall short of providing the answer, although we may be near to developing some measurements sensitive to degree of asymmetry.[22] As we learn that a number of measures will discriminate what appears to be "degree of asymmetry," we can proceed to name those qualities of the image which seem to index direction and size of asymmetry. When we have done so, the relationship of any measure to these indexing qualities represents the criterion validity of a measure of asymmetry. The correlation coefficients among these indexing measures are concurrent validity coefficients.[23]

Promising Areas for Development of Measures

This paper has advocated an eclectic, initially descriptive science of visual communication, one which gradually defines its area of concern with an inventory of sensitive, reliable and relevant measures. The gradually unfolding science will posit a set of principles which describe each of the various dimensions of visual imagery which can affect the perceptions of an audience. At the moment the bases for such a science are fragmentary and tentative. It is certain that this science will have to explain both the social and personal significance of visual elements, the contributions of viewing environment and program context as well as the influence of viewer personality and culture.

More scaling procedures need to be explored in developing relevant measures of image dimensions. Some measures must be extremely sensitive, sensitive enough, for example, to discriminate responses to a symmetric image of a symmetric object and responses to an asymmetric image of a symmetric object. More needs to be done with physiological instrumentation,

considering the extreme sensitivity of which these measures appear capable. In addition, the continuous nature of physiological recording offers the promise of monitoring moment-by-moment response to a string of changing visual compositions.[24]

But, more important than new measures and new procedures, new curious eyes and ears need to scan the exotic terrain of visual communication, finding in it not only a Shangi-La for mystic contemplation and scholarly rapture -- but a rational encyclopedia of inductively based generalizations which, when mastered, offer new views of human capacity to share both personal insight and collective dream . . . how we got to be this way . . . how we are shaping the visual universe . . . for our posterity . . . and for our own maturity.

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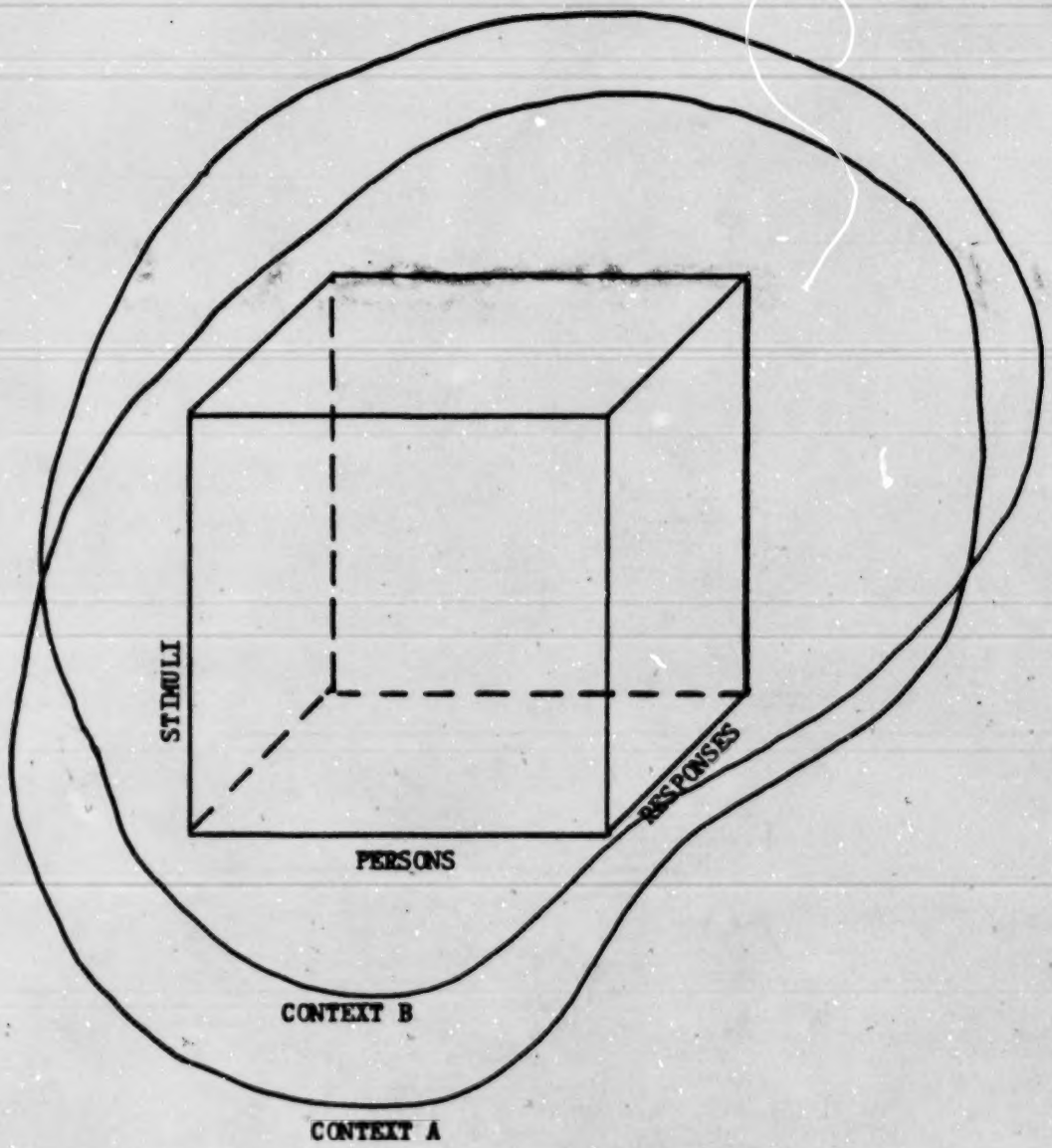

Figure 1. Variables in a behavioral study.

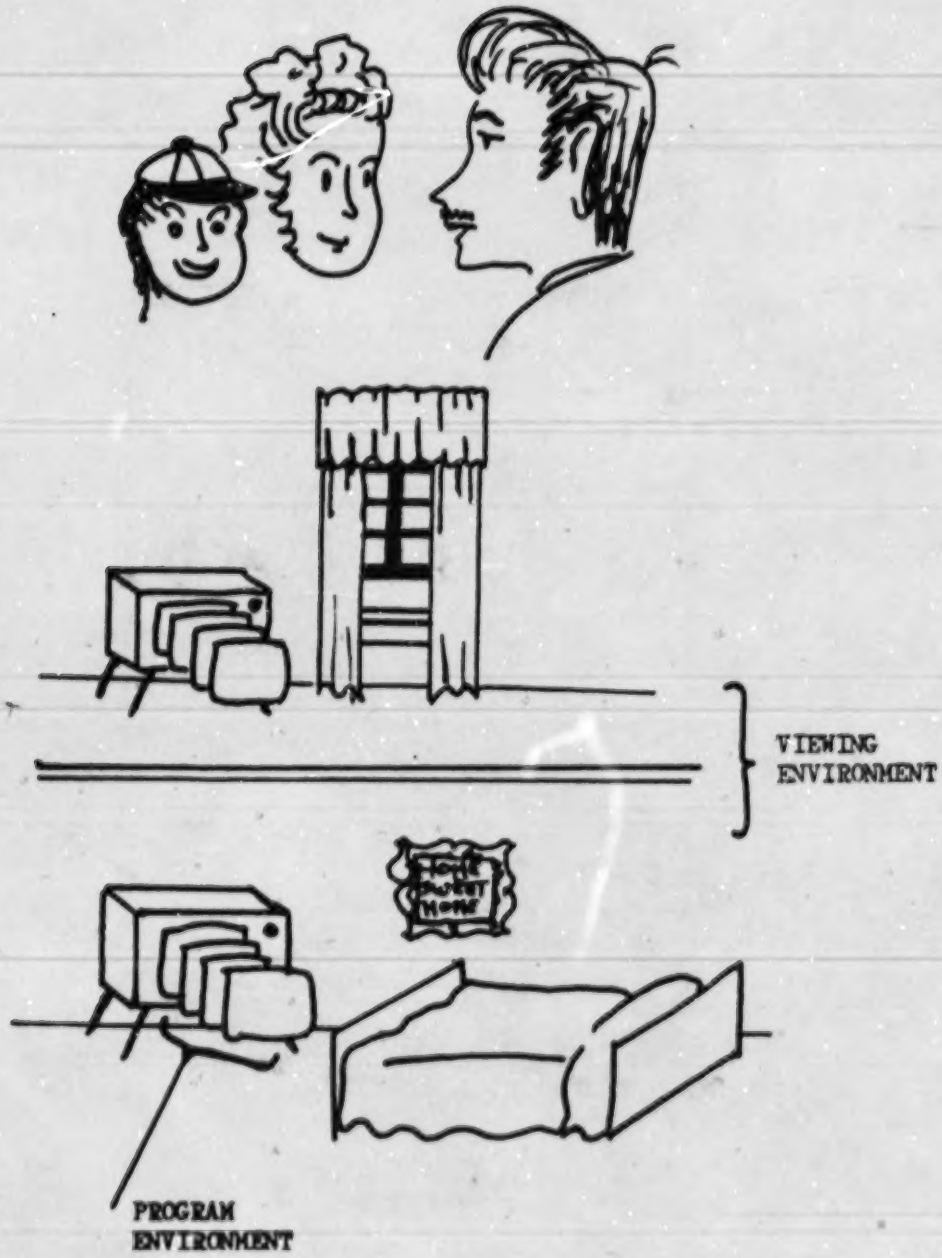

Figure 2. Viewing and program environment.

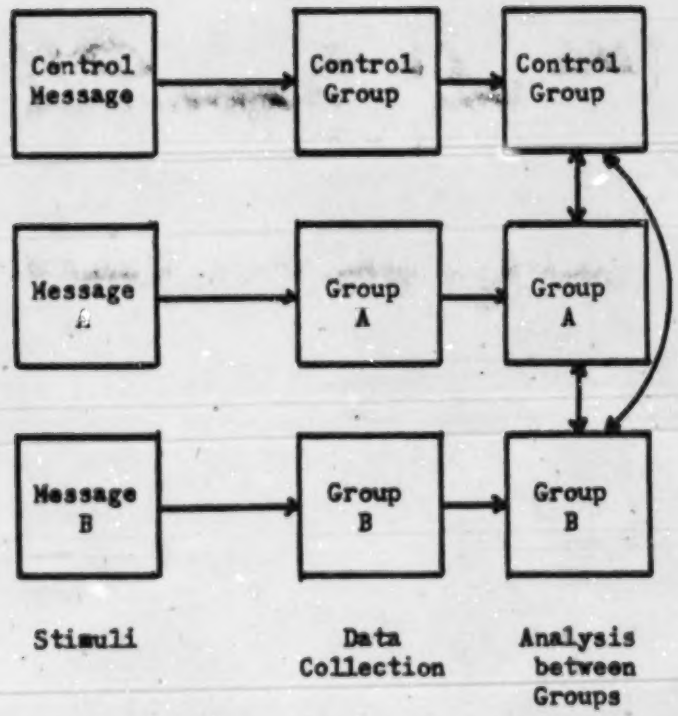

Figure 3. Typical factorial study design.

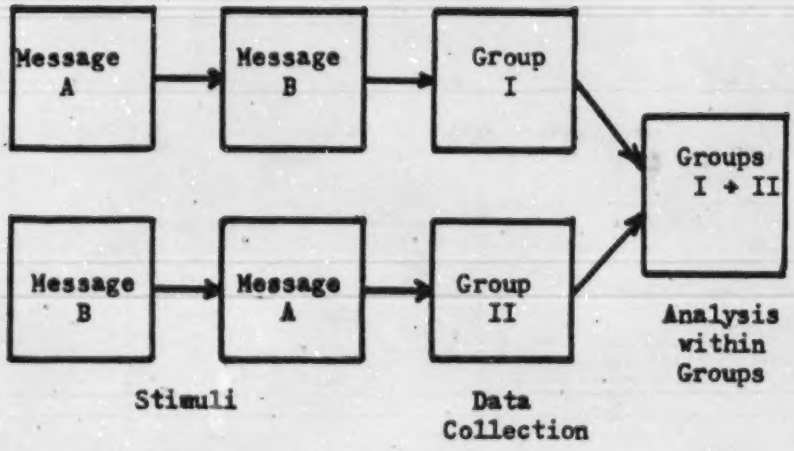

Figure 4. Repeated measures study design.

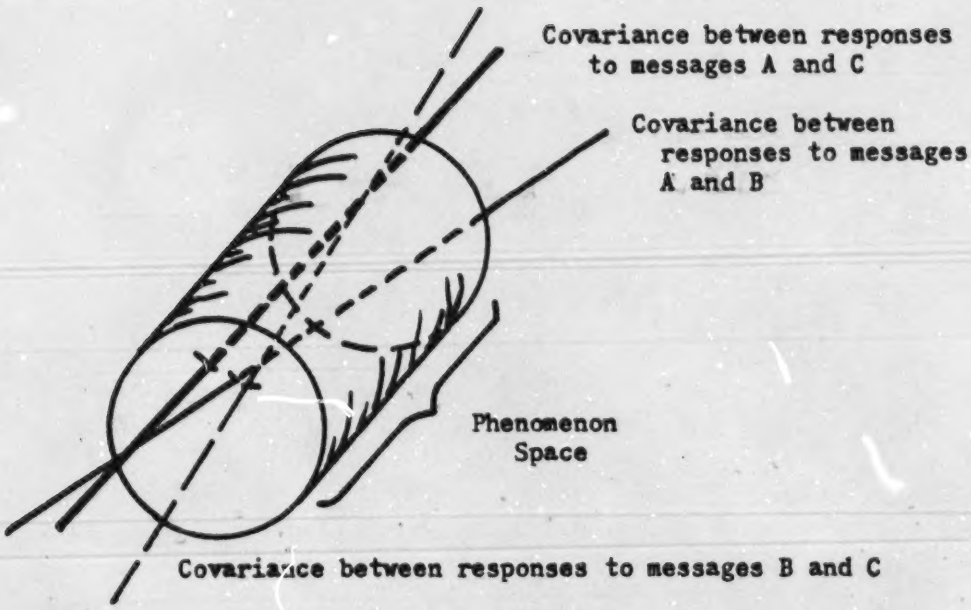
Covariance between responses to messages A and C
Covariance between responses to messages A and B
Phenomenon Space
Covariance between responses to messages B and C

Figure 5a. Homogeneity of covariance in a repeated measures study.

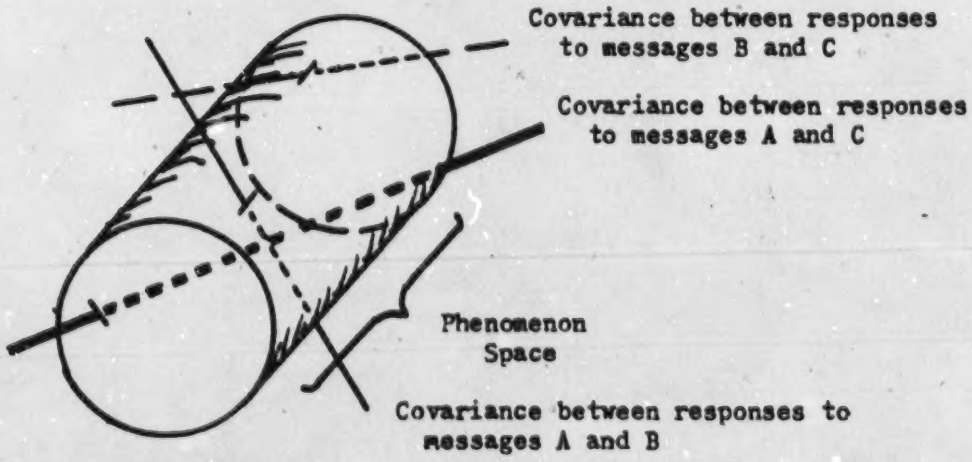
Covariance between responses to messages B and C
Covariance between responses to messages A and C
Phenomenon Space
Covariance between responses to messages A and B

Figure 5b. Heterogeneity of covariance in a repeated measures study.